

# Human Ethology Bulletin

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## An Interview of Robert Trivers

Text: Frans Roes

Photograph: Alex Walter

Robert Trivers became well known for his publications on, among other things, parental investment and sexual selection, parent-offspring conflict, and reciprocal altruism. The following interview took place during the conference on 'Biological Perspectives in the Social Sciences' at the Gruter Institute, Dartmouth College, Hanover, NH, USA August 1995. A slightly different version of this interview was published in the Dutch newspaper *NRC-Handelsblad*, Nov 16, 1995.

**Thirty years ago or so your life was turned around by evolutionary theory. How did this happen?**

I was a history major in college, after having been in mathematics. Of course I heard of Darwin, but I had never had any biology. I had never watched animals or paid any attention to them growing up as a child. I remember people at the university used to make fun of me, because they would show me a picture of a rhinoceros and I might guess it was a hippopotamus—I didn't even know my animals! I believed, like many people then, that human behaviour had very little in common with animal behaviour—with of course no knowledge of this subject at all. When I was 22 years old, I was asked to write children's books on animal behaviour, and I became exposed to facts about animals. I remember being struck by a very good movie footage on adult-offspring interactions in baboons. The adults indulged in something that looked like parental discipline of young baboons that were beating up other youngsters. It reminded me very much of parental discipline in our own species, the big difference



being that the baboons said nothing while disciplining their youngsters, while we of course fill the airways with words. So that immediately suggested that parental discipline did not require language, and if it didn't require language, you needed an explanation that applied to both baboons and humans at the same time. And that led naturally into evolutionary logic, because it is only evolutionary logic that is going to provide us with an explanation that works for many different species.

**Speed and strength are important adaptive traits. Then why are not all living organisms fast and strong?**

Well, all traits in principle have a drawback and negative features which in some settings outweigh the positive ones. Strength for example is costly in protein necessary to build and maintain muscle mass. So the expensiveness of strength can easily outweigh its benefits in various situations. No trait is adaptive in all environments.

You are reported to have said: "All you need to know is Darwin and Hamilton". What did you mean?

The only change in our conception of natural selection since Darwin is the result of Hamilton's work. All you need to know to understand the underlying evolutionary principle of natural selection is Darwin for giving us the concept of fitness, or reproductive success--number of surviving offspring--and Hamilton who extended this to effects on other relatives. He noted that we are not only related to our children, we are also related to our brothers and sisters, more distantly our cousins and so on. So Hamilton came up with a slightly more general formulation which says that we are not trying to maximize the number of surviving offspring per se; no, we are trying to maximize the number of surviving copies of our own genes, whether found in offspring or found in other relatives, each category weighted by how closely related we are, or, as we call it, degree of relatedness.

You introduced the term 'reproductive success' to replace 'fitness'. Why?

I very much dislike the tendency in academia in general to proliferate unnecessary terms, and yet I am someone who has done that. Prior to 1970, I don't think the term reproductive success was much used, but instead the term fitness was used. I did not like the term fitness because of its connotation of being physically fit. It suggested that you could tell who was fit before you found out who left many surviving genes. Over the preceding hundred years, ever since Darwin, fitness was used in this dual sense, and people slipped back and forth between fitness meaning simply reproductive success, or something that could be judged separately, like physically fit. So I coined the term reproductive success simply because it was more accurate. It caught on, and yet Hamilton who preceded me had already chosen 'inclusive fitness', and nobody, including me, uses 'inclusive reproductive success'. So we have parallel language usage. I do like the term reproductive success though, and I have no doubt that in teaching students it is beneficial to use that term, and not to use the term fitness.

In your book *Social Evolution* you describe social differences between seals breeding on land or on ice, different sex ratios between ants

that do or don't hold slaves, and many other examples from the animal kingdom. Why should studying these phenomena be relevant to social scientists who are interested in human behaviour?

It is very important what the form of the argument is linking other creatures to humans. One thing we are trying to do is understand general theories that apply to our own species but also apply to other species, and it is often easier to test the general theory in some other species than in our own. That often makes studying distantly related creatures valuable to understanding ourselves, not because we act like them, not because we necessarily share any behaviours in common, but because we are both subject to the same principles. And to test and refine the principles themselves, it is valuable to get away from humans. To give you an example, when I first worked on parent-offspring conflict, Richard Alexander said: "Well and good. There is parent-offspring conflict in theory. But if you go to nature you will find that the parent always wins. The outcome is always exactly what the parent wants". Now for our own species, there is no way to measure the relevant parameters sufficiently precisely so as to test that notion. The cost of an additional day of nursing an offspring? Very hard to measure. The benefit of a given day of nursing to the offspring? Difficult to measure. Remember cost and benefit must be expressed in terms of reproductive success. However, you could go to those ants you were mentioning, and you could prove back in the seventies that regarding some parameters in an ant nest, the offspring wins, and the mother loses. Now you cannot generalize from that result to say: "Oh well, in humans the offspring always wins". Nonsense! In ants the mother is facing tens of thousands of daughters simultaneously. But the demonstration that offspring are capable of expressing their own interests counter to their parent's best interests destroyed a certain line of reasoning regarding the general principle.

Are there aspects of social behaviour of any species for which evolutionary theory is irrelevant?

I cannot imagine there are. I am a little timid about work on humans recently, because I have not concentrated on that for some time, and because people care so much about the

application to humans, get emotional about it. I would prefer to speak from a position of strength, and not from a position of weakness or ignorance. You know, the questions often get subtle and complex, and if you are not on top of every nuance and detail....

**A polemical statement: Most social scientists are either anti-Darwinist, or only have misconceptions about evolutionary theory. For that reason they are trailing some 140 years. Do you agree?**

Well in this country, that is, the US, I feel that this is a fairly accurate picture, but I don't know about social scientists the world around. Here most social scientists, as part of their training, learn reasons why biology is irrelevant. For instance, anthropologists learn that culture is critical and not shared by any other creature, so forget about all the rest of the creatures. By the way, the most distressing feature of this to me is the failure to educate the students in some biology. I harp on this educational thing, because until that has changed, you are continuing to turn out a generation of people who will be ill prepared to understand and accept biological work being done in their area. Let's say you are a forty year old psychology professor, and you come on Darwin, and you come on Hamilton and some recent work in evolutionary psychology, and you say: "My God, that looks exiting and fun". Now if you have never had a course in biology, there is so much work staring you in the face before you feel you can be expert in this area so as to use it, that there will be a very strong tendency for you to do the opposite: Figure out reasons why evolutionary theory is not relevant and not so important. So this failure to educate the graduate students in a little bit of biology is a very regressive feature of the educational system, and it slows down the movement of biology into the social sciences. The social sciences are divided in all these subsections that do not get properly integrated and related to each other. You know, twenty years ago I had the future exactly wrong. I didn't imagine the work we were doing taking over biology to the degree that is has. I instead imagined that the social sciences would be reformulated around this work. I confidently predicted that in twenty years, in other words right now, you would not be able to walk down the hall of a psychology or a sociology or an anthropology department without hearing

people arguing with each other, "Yes but why would natural selection favour that?" This has not happened. The parallel fact in biology has been extremely gratifying, for instance completely reorganizing the work on animal behaviour, and I was surprised by it. But biology is a unified science with a central paradigm coming out of Darwin, so it is much easier for ideas to rapidly diffuse within biology.

#### Bulletin Submissions and Duplication

Anything that might be of interest to ISHE members is welcome: Society matters; articles; replies to articles; suggestions; announcements of meetings, journals or professional societies; etc. These sorts of submission should be sent to the editor. Book review inquiries should go to the appropriate book review editor (Linda Mealey, the chief book review editor, covers books in English). Submission should be in English, on paper and, if possible, also on diskette (MS Word 5.0 preferred). Shorter reviews are desirable (less than 1000 words). **Please include complete references for all publications cited.** For book reviews, please include publisher's mailing address and the price of hardback and paperback editions. There usually is not time to consult with reviewers about editorial changes, but most of these are minor.

Submissions are usually reviewed only by the editorial staff. However, some submissions are rejected. Political censorship is avoided, so as to foster free and creative exchange of (even outrageous) ideas among scholars. The fact that material appears in the *Bulletin* never implies the truth of those ideas, ISHE's endorsement of them, or support for any policy implications that may be inferred from them.

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## SOCIETY NEWS

### *Election of Officers*

The ballot for election of the following two officers appears below: Vice-President/President-Elect, and Treasurer. Please complete it (or a photocopy) and send it to the ISHE Secretary, Karl Grammer. Deadline is 1 September. Where only one nominee appears for each office, no other names were submitted; in such cases, however, members are free to write in the name of someone else. Terms of office are three years. The Vice-President/President-Elect automatically succeeds to the Presidency at the expiration of the latter's term. Thus the current incumbent, Charles Crawford, will succeed Bill Charlesworth as President at the end of 1996.

### *Membership Directory Corrections*

Correct addresses for B. B. Houx:

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Correction for Kathryn Coe:

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Correction for Tom Shellberg:

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Revised listing for Lee Cronk:

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E-mail: L-Cronk@tamu.edu

### *Correction*

In the March 1996 *Bulletin*, an error appeared in Frank Salter's review of Philippe Rushton's *Race, Evolution, and Behavior*. The first sentence in the last paragraph on p. 19 should read: "To give an idea of the breadth of this volume, consider Chapter 5, which presents a necessarily potted [sic] history of the race concept and of racism." The error was the fault of the editor, who apologizes to all concerned.

### *Bulletin Subscriptions*

As the application states, memberships in ISHE and, hence, *Bulletin* subscriptions, are by calendar year. If you wish your subscription to start the next calendar year rather than the current one, please so indicate on your application.

Another note: From time to time someone receives a request to renew a subscription that is paid up. The editor apologizes for these errors, and asks that you kindly bring them to our attention. We are hoping to begin sending invoices out on a regular basis; this should reduce such errors.

### BALLOT FOR ISHE OFFICERS

#### Vice-President/President-Elect

Linda Mealey, Dept. of Psychology,  
Queensland University, Brisbane, Australia ....

Other (write in name and affiliation) ....

\* \* \*

#### Treasurer

Barbara F. Fuller, University of Colorado,  
School of Nursing, Denver, Colorado, USA ....

Other (write in name and affiliation) ....

Please mail ballot to Karl Grammer, Ludwig-Boltzmann-Institute for Urban Ethology/Human Biology, Althanstrasse 14, A-1090 Vienna, Austria, fax 43-1-31-336-788 by 1 September 1996.

## Vienna Congress Epistemology Workshop Topics Requested

The position of human ethology as a distinct scientific discipline securely established within traditional institutions has, with few exceptions, not been fully realized. In 1975 E. O. Wilson saw ethology (and comparative psychology) as "destined to be cannibalized by neurophysiology and sensory physiology from one end and by sociobiology and behavioral ecology from the other...." Many of us think this will not and can not happen--at least not for human ethology in particular. However, that the name of the journal *Ethology and Sociobiology* will most likely be changed to a new title which will not contain "ethology" in it suggests that there may be some substance to Wilson's prediction.

In light of this and other indicators, it is a good time to take a look at ourselves and at what we see as our distinctive contribution to knowledge of human behavior. The workshop "Epistemology" aims to take this look--not by insisting that human ethology is a good thing (we know that), but by examining the nature of the knowledge human ethologists seek.

The workshop will concentrate on major themes in ethology as expressed within the framework of the following questions:

- What knowledge do human ethologists seek that is distinct from that sought by other scientific disciplines, and what is the best way to acquire this knowledge?
- What are the main problems in acquiring this knowledge?
- How does this knowledge relate to evolutionary theory?
- How does this knowledge connect with other disciplines?

The workshop leaders--W. Schleidt, J. Richer, C. Crawford, and B. Charlesworth--will center their opening comments on:

- Naturalistic observation, description, and the ethogram
- Ethology's "four questions"

- Applying ethology to the study of cognition and the neurosciences
- Human ethology's connections with behavioral genetics, ecology, cultural anthropology, and social psychology
- The place of evolutionary theory in human ethology research
- Human ethology's contribution to studying problems of human social adaptation.

The workshop leaders invite ISHE members to submit topics they want to be discussed. We hope there will be time to include all submissions. A summary of the workshop will appear in this *Bulletin*. Please submit topics to: Bill Charlesworth, Institute of Child Development, University of Minnesota, Minneapolis, MN 55455 USA, fax 1-612-624-6373.

## Missing Members

If you can inform us of the current address of any of the following, please let us know:

Catherine E. Hill, formerly of Durham, NC, USA.

Sharon R. Bidwell-Cerone, formerly at the University of Rochester, Rochester, NY, USA.

Karen Olson, formerly of 207 Pond St., Natick, MA, USA.

## French Book Review Editor

Peter LaFrenière has asked to be relieved of his position as French Book Review Editor for the *Bulletin*. He has moved to the University of Maine, and has been busy preparing a review of recent books on emotion. If you read French and reside in a Francophone location, please consider providing this service to us. We shall continue to list Peter in this role until he can be replaced, but that depends mainly on someone else stepping forward. Peter's new address appears in the Staff box.

## BOOK REVIEWS

### *Human Facial Expression: An Evolutionary View*

By Alan J. Fridlund. Academic Press, San Diego, CA 92110-4495, 1994, \$ 59 (hdbk.)

Reviewed by Alain Schmitt, Ludwig-Boltzmann-Institut für Stadtethologie, c/o Inst. für Humanbiologie, Universität Wien, Althanstr. 14, A-1090 Vienna, Austria

This text presents an almost perfect overview and meta-analysis of the state of the art and beyond. It covers the historical, logical, methodological, biological and psychological bases of extant research on facial expression, and proposes a new theory and a host of derived hypotheses and ways to test them.

The book has 13 chapters. The first two give a brief history of physiognomology. They show that from Aristotle to Darwin and the 20th century, theory has and does always influence both observation and the selective quotation of predecessors' data and arguments. Chapter 3 starts a series of methodological arguments demonstrating the shortcomings of existing research and theories. It then axiomatically lists epistemologically sound strategies for the future and develops a new theory, the "behavioural ecology view of facial expression." These topics also form the bulk of chapters 6, 7 and 11 to 13.

Chapter 5 summarizes what is known about facial hardware, i.e., nerves and muscles. Here is an omission worth mentioning: Eibl-Eibesfeldt (1984, 1995) presents evidence that there is quite great intra- and inter-racial variability for some facial muscles (e.g., *musculi risorius* and *zygomaticus*), although all these different people allegedly produce similar and recognizable smiles.

Chapter 6 describes facial reflexes (sneezing, blushing, yawning, etc.) and the phylogeny and ontogeny of facial displays. Fridlund cites neurological evidence to corroborate his thesis that eyebrow-raising and -knitting are phylogenetically derived from protective earflap retraction (startle reflex)

and auditory orientation (surprise), respectively. This runs counter to the usual interpretation of brow movements as vision regulators. Cranial neoteny seems to have led to this functional shift. Consequently, our eyebrow movements are not homologous to those of non-human primates.

Chapters 8 to 10 finally present the evidence for the universality of facial expression and recognition of emotions. Chapter 10 is a reprint of J. A. Russell's (1994) review paper on cross-cultural studies, which spawned a vehement debate with Ekman (1994) and Izard (1994) exactly at the time when Fridlund's book appeared. This is an interesting coincidence, since Fridlund planned a "universality" chapter for his book, but then became aware of Russell's review and reprinted it instead of publishing his own version. None of the papers refers to Fridlund's forthcoming book, even though he has published with both Izard and Ekman. Personally, I was more impressed by Russell's and Fridlund's critiques than by Ekman's and Izard's replies.

To put my (meta)review of the universality thesis in a nutshell: There is a consistency of results spanning 70 years of research and showing that people all over the world can produce and recognize at better than chance level, but with substantially less than perfect accuracy, specific facial "movement" patterns. "Recognize" means that people can associate posed (non-spontaneous) still pictures with broad clusters of words centering around happiness, anger, disgust, surprise, fear, and sadness. This does not say much on the question of whether facial expressions are everywhere manifestations of similar feelings or emotions. Recognition accuracy varies rather largely with culture, degree of exposure to Western civilization, and education level within a culture (disgust and sadness were particularly unreliable categories; happiness is a notable exception to the rule). By and large, people are best able to distinguish faces connotating "positive" and "negative" situation or emotion terms. Epistemologically, it is particularly interesting to note that these results emerge from a set of studies specifically designed to demonstrate universality. Whereas random choice has thus been ruled out cross-culturally by past studies, future studies have to become more ecologically valid, that is, real life behaviour in natural settings has to be studied.

Fridlund, however, argues that even a perfect finding of universality would never allow a definite answer to the innate/genetic vs. learning hypotheses, a goal that the proponents of cross-cultural research on facial expression have repeatedly claimed to have reached (see Izard's paper). In fact, the universality and innateness/genetic control issues are quite independent from each other. This is an example of one of Fridlund's penetrating logical analyses. His arguments go to the principles of the comparative method and are as follows. (1) Innateness does not require universality; it suffices that a behaviour only be present in most cultures. (2) Commonalities may reflect either innateness and shared phylogeny, or convergent evolution, or learning common to all. For example, universals such as belief in god(s), analogs of Oedipal problems, and tongue protrusion to expel food may everywhere be caused by common life constraints. (3) Non-universality does not exclude innateness. Indeed, differences among cultures may be due to genetic drift and founder effects, divergent selective forces, or culture. For instance, skin colour and blood types vary across ethnicity, but they are very probably not acquired by tradition.

Fridlund proposes that the most valid inferences from the comparative method would stem from the following facts: A similar display is observed in two species (or cultures) and is advantageous to individuals of the first, but not to individuals of the second. This would clearly show that the behaviour is vestigial in the second species/culture, and thus under genetic control.

In sum, there are many alternative interpretations of cross-cultural commonalities, ranging from entirely genetic to completely cultural explanations. Cross-cultural similarities may indicate a phylogenetic origin of a display, but alternatives have to be ruled out. Various types of analysis are useful for reaching a conclusion: adaptive utility comparisons, cross-species comparisons, detailed and quantitative comparative analysis of display forms, and neonate and twin studies.

Finally, let me try to summarize the two opposing theoretical positions currently dominating the field of facial expression: the "emotions view" (EV, basically Ekman's

"neurocultural model") and the "behavioural ecology view" (BEV, also called communications view or interaction view by Fridlund). EV sees facial expression as the involuntary readout of modular affect programs. Internal, discrete and innate emotional states (less than 10 states) combine with social elicitors to yield prototypical facial gestures that are recognized as such by all humans. Ontogenetically, displays are trained to become voluntary and disguisable, i.e., culture-specific "display rules" are learned, and displays then may be used in non-emotional contexts. The results are "felt" and "false" faces (e.g., 18 felt and one false smile, Ekman [1992]), and blends of emotions and faces (e.g., angry and sad).

By contrast, BEV sees facial displays as declarations of intent within a specific social context, as social tools used to negotiate interaction and to signal social inclination. Each display family (smiles, eye-brow movements) serves a whole cluster of intentions and contexts, and in some occasions may be accompanied by and express emotion. Meaning arises only in the context of occurrence, and no distinction is made between felt and false displays.

Fridlund convincingly argues that the evidence favors BEV. He has four central arguments:

(1) Cross-cultural studies have shown that recognition of facial expressions is only relatively universal (see above).

(2) There is a poor relationship between emotion and facial displays: (a) The facial expressions of neonates and adults in response to odors and tastes do not track the hedonics of the stimuli. (b) The large majority of everyday life facial gestures are displayed during speech, where they do not connote emotion at all. Both Ekman's and Fridlund's research concluded that less than one-third of facial displays convey emotion; thus the analysis of facial expression of emotion is based on a minority of all facial displays occurring in daily life. (c) When given free choice in labeling photographs of allegedly emotional faces, a vast majority of people do not give emotion terms, but rather situation examples.

(3) Display frequency depends on who and how

many individuals are present, both in animals and in humans (audience effects, social facilitation).

(4) EV fails to give inclusion or exclusion criteria for what constitutes an emotion. For example, surprise in itself is anhedonic. In the EV it is one of the basic and unitary emotions, but in the BEV it communicates cognitive uncertainty and readiness to investigate.

Fridlund's main examples to contrast the two positions are happiness and anger. In contexts in which one would try to appease another, smiles would be labeled "false" by EV, but BEV would label them "about to appease" or "I give in" displays. In fact, smiles occur not only when we are reconciling, but also when we are polite, flirting, loving, embarrassed, etc. Thus, smiles can reflect love, anger, et al., and not just happiness, the fundamental emotion associated with smile in EV. Moreover, we smile a lot during flirtation, but much less during foreplay and coitus, even though we (presumably) enjoy the latter more. EV, however, would predict more smiling during coitus.

Threat displays may be reinterpreted similarly. The BEV would predict that we do not display angry faces when we physically attack an opponent, but we do when we debate and want to express our intent and to harm. On the other hand, EV would predict that we do display angry faces during an attack.

The book is a profoundly scholarly review with an illuminating prospect. It is a must for all interested in communication, especially those involved in research on human interaction. It is up to date, richly and well illustrated, written with exceptionally clear language and logic, and particularly penetrating and disturbing.

#### References

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### *Touch: The Foundation of Experience*

Edited by K.E. Bernard & T.B. Brazelton.  
International Univ. Press, Inc., 8920124  
Madison, Connecticut (Clinical Infant Reports  
No. 3), 1990, \$55 (hdbk.).

Reviewed by Anton Furlinger, Isbarygasse 13,  
Vienna, A-1140 Austria.

There is no question that the visual sense dominates our experience in everyday life. And, no doubt, most laureated neuroscientists have worked their way to distinction through study of the visual pathways. Maybe, as Churchland et al. (1994) claim, we need "a critique of pure vision" in order to take intermodality more seriously and view behaviour as *both* sensory and motor. This welcomed book begins this process with a focus on the importance of touch as "the foundation of experience." Touch is the first sensory modality to emerge in ontogeny, and the most visible, accessible, and largest of all vertebrate organs is the skin. (Together with the second, the vestibular senses, this may suggest a *relational* origin of the nervous system: that is, the vestibular sense monitors one's relation to the earth's "body," while touch monitors the relation of self to other bodies impinging upon the skin.)

This monograph succeeds in integrating work from neuroanatomical, comparative biological, sensory-motor, learning, and developmental viewpoints. It covers the entire life span with an emphasis on preterm



infant care and on therapeutic applications in general.

The introductory chapters are as follows: (1) Merzenich's famous experiments on the plasticity of the somatosensory cortex show that experience not only lays the foundation of cortical maps (possibly beginning in utero?), but alters them throughout life. (2) Diamond and Greenough describe how the somatosensory development of rats reared in enriched, complex environments results in gross enlargement of brain tissue and neuronal dendritic fields. (3) Suomi presents an evaluation of short-term social separation experiments with young rhesus monkeys, showing how even brief separations can lead to systematic problems in social development, thus adding evidence to Harlow's doctrine of the tactual nature of "love." (4) Levine and Stanton corroborate this by arguing that contact between mother and infant (in their case squirrel monkeys and rats) may be a crucial mechanism to modulate level of arousal in both infants and mothers. (5) Reite, using data collected by an implantable biotelemetry device, examines the physiological correlates of agitation and of depression seen in young pig-tail monkeys after maternal separation. Touch is shown to have regulating or signalling influences on physiology which can promote attachment and health. He asserts that if we can understand the biology of a behavioral system, then psychology will make more sense. He is right!

Taken together, these chapters attribute to the tactile mode the role of an early integrator for the other senses--even if the visual sense later emerges as hegemonious. In the next section of the book, a wealth of information about work in intensive care nurseries is presented and evaluated (Gorski, Rausch, Körner). For example, the fetuses of comotose mothers are not rocked--is that a problem? The sensory ecology of a fetus and the design of an environment permitting diagnostic and therapeutic activities are not easy to reconcile; rocking, stroking and water beds for the preterm infant are only starting points. Furthermore, we do not know the rhythms a fetus can or must track to develop its nervous system.

Next comes a chapter by Satz ("A developmental study of finger localization and reading achievement"), followed by one by Gottfried, who reviews the role of touch in early development. At one hundred days, the fetus is sensitive to touch all over the body "except for the top and back of the head, which remain insensitive until birth." According to Gottfried there is still "a paucity of data on the amount and nature of tactile contact of premature infants in special care units." "Therapeutic touch" is next introduced by Meehan, specified for midwifery by Wolfson, and critically appraised by Smith. For me, as a physician, reports that touch can change blood composition even without direct contact are hard to believe; on the other hand, isn't every (pro)therapeutic interaction characterized by extra-ordinarity in ethological, natural terms?

Let us try to model a "healing situation," the atmosphere of which evokes an "aura curae" for the patient. [Langer (1987) used this term to broaden the context of placebo effects.] Imagine, first, a sick individual realizing that another (unfamiliar) individual is approaching closer than the "flight distance" even a curious mammal would keep from a deviant (manifestly sick) conspecific. Then, intentional "manipulations" are administered *without* any clear metacommunication signifying ordinary motives such as altruism, threat, or sexuality. The sick individual's body cannot react in a normal, contingent way--how does it react?

In the last section, on the life spectrum, Weiss shows how meaning is created in cognitive maps. She argues that learning through tactile stimulation is a necessary precondition for learning via other modalities (nicely paraphrasing the title of the volume). She then examines the effect of parental touch and arrives at interesting and unexpected sex differences. Next, in her "Language of touch," Main demonstrates that parental aversion to infant-initiated contact is transmitted across generations. McAnarney rightly points to the importance and risks of tenderness in adolescent interactions. "Since women are usually held or cuddled before or after coitus, they can use sex as a means to get this type of body contact." For a more elaborate discussion of the "tender"

quality of touch see Koortmulder (1994). The closing, philosophical chapter concerns the history of pertinent concepts.

Scattered among the chapters are lively discussions. However, some crucial questions are posed but not answered. Overall, the book can be strongly recommended for physicians, nurses, and graduate students in behavioral biology, (behavioral) physiology, and developmental biology.

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### *Evolutionary Ecology and Human Behavior*

Edited by Eric Alden Smith and Bruce Winterhalder. Aldine de Gruyter, 200 Saw Mill River Rd., Hawthorne, NY 10532 USA, 1992, \$29.95 (ppr.), \$59.95 (hdbk.).

Reviewed by Richard Pocklington, Dept. of Biological Sciences, Simon Fraser University, Burnaby, B. C. V5A 1S6, Canada.

The goal of this book is to provide a coherent advanced textbook for studies in human evolutionary ecology, while including research-quality articles. All in all, the attempt is a success. However, the hybrid nature of the project leads to a trade-off which makes it neither a full fledged textbook nor a repository of unique primary research. It does provide a good introduction to human behavioural ecology, as well as acting as a great resource for those seeking more advanced information. Serving a purpose similar to Krebs

and Davies' *Behavioural Ecology*, it will be a valuable addition to the bookshelves of anyone interested in human behaviour.

As a broad review of human evolutionary ecology, it acts to point out the strengths and the weakness of the approach. Although there is some discussion of the multiple levels of causation of behaviour, throughout the book very little attention is devoted to anything but the ultimate, or survival value, approach. A more pluralistic approach might have paid off. Mechanistic description often helps guide evolutionary explanations, and vice versa.

One thing struck me while reading this textbook that overshadowed all other reservations I had with it. For some time now there have been overdrawn criticisms of those who import models from the natural sciences. The typical critique runs like this: (1) Science is a social enterprise. (2) Social values thus influence how science is done, in particular the theme or framework in which we study things. (3) A standard problem with importing natural science into social science is that the ideas which are imported have their genesis in social theory. These ideas leak into science and back again, perhaps due to the fallible nature of scientists or to fundamental limitations to the scientific method. (4) Thus some ideas that are imported to social science from natural science have been recycled, so to speak. These ideas have now been 'naturalized' through their stay in the hard sciences. This lends credence to them, and thus social ideology may become policy through a circuitous route. The use of economic models in anthropology seems to be a clear example where this type of roundabout information leakage may be important.

I would wish to qualify the 'social leakage hypothesis' by noting that interdisciplinary modeling may truly reflect general processes that are shared amongst disciplines. Thus, to import a model from the social to the biological sciences may not necessarily be a bad thing, if it is done right. By done right I mean that the visit to the natural sciences should harden the ideas and clear away some of the misconceptions that may have remained while the theory lay without access to a system that permits rigorous

experimental testing. For example, the change from the vague and tautological definition of utility to the more concrete notion of fitness may be just what an economic theory of human behaviour requires.

Thus we must be sure that the economic theory that was generated in the social sciences and later adapted to the use of evolutionary biologists is not tainted with antiquated social models when we make use of it in human evolutionary ecology. We must be careful that the use of biological models is always based on solid, empirically tested relationships. Interdisciplinary borrowing can be incredibly useful. However, it is dangerous to grab models out of context and use them without a firm knowledge of their assumptions and limitations.

We should be aware that much of the territory now under the purview of evolutionary ecology has been explored by economists. Reading the original sources rather than the version filtered through animal ecology may be essential if we wish to delve into the 'phylogenetic roots' of our current models. We should be particularly cognizant of the fact that all things are a product of an interaction between their current environment and their developmental and evolutionary history. Our theories develop and evolve, and I would strongly recommend that evolutionary ecologists spend time exploring the conceptual roots of their discipline. Only then may we discover what historical constraints are placed on our ideas and thus transcend them to produce a new generation of clearer and more objective science.

The criticisms I have raised here are dealt with, to a degree, by the editors. They devote a good portion of the book to a description and justification of their methodology. They analyze the relationship between evolutionary ecology and other ways of studying human behaviour, and they address many challenging criticisms of their field.

While the defense of the methodologies used in the book is coherent, I would have preferred one that referred to the data more explicitly. When engaging in the rhetoric which clouds almost all aspects of the study of human behaviour, it is important that those who take a scientific approach ground

themselves in their data. Verbal arguments are one thing, well-tested hypotheses another.

The editors also discuss the supposed political implications of their work. They point out that there is no clear connection between conservative political stances and biologically based theories of behaviour. Lysenkoism and degenerate forms of Skinner's behaviorism are prime examples of totalitarian anti-hereditarianism.

The topics dealt with in *Evolutionary Ecology and Human Behavior* are Theoretical Foundations: natural selection and cultural inheritance; Primate Social Structure; Time Allocation; Social Hierarchies; Fossil Hominids: habitat use and reproduction; and Food Acquisition: collective action and population dynamics.

The section on cultural evolution is a bit out of place when compared with the other chapters. It presents a solid, but basic, introduction to the cultural evolution theories that have evolved in parallel with sociobiological theory. It was a pleasant surprise to find it in this textbook; it is definitely the most conceptually novel area of study covered. Cultural transmission theory, like population genetics, deals primarily with mechanisms of change across generations, while evolutionary ecology concentrates on the analysis of survival value. This digression from the constant focus on the self-centered, optimal individual is refreshing. Unfortunately, there is a dearth of empirical work in this area.

The bulk of the chapters deal with specific topics in evolutionary ecology, and present an introduction to the nature of the problem, a simple mathematical model, and evidence used to test predictions from the model. This was mostly an honest treatment. Evidence both for and against the theories are presented and dealt with clearly, and the conclusions are not overly ambitious. The strengths and weaknesses of the particular approaches and data sources are examined.

Much of the information could have been presented in a more appealing format. There were not enough summary boxes and graphical examples in some chapters. One of the things that keep the attention of readers of

books on animal behaviour is the fascinating illustrations and photographs. The editors of this text may have gone out of their way to make sure it had a dry, academic feel. I think that there is much educational value in providing at least black and white illustrations.

I think that the editors made good choices of chapter material, but the integration between the chapters could have been improved. The hominid prehistory and comparative primatology sections stood out as particularly disjointed. They could have been much better integrated into evolutionary ecology.

Integrating these bodies of disparate evidence will be difficult, but it is essential if we are to have a comprehensive picture of the evolution of human behaviour. This book takes a good step forward towards such a comprehensive, integrated study of the evolution of human behaviour patterns.

### *Tools, Language and Cognition in Human Evolution*

**Edited by Kathleen R. Gibson and Tim Ingold.**  
Cambridge University Press, 40 W. 20th St.,  
New York, NY 10011 USA, 1993, hdbk. and ppr.

**Reviewed by Thomas R. Alley,** Department of  
Psychology, Clemson University, Clemson, SC  
29634-1511, USA.

This volume resulted from a 1990 interdisciplinary conference on "Tools, language and intelligence: Evolutionary implications," held in Portugal. The conference organizers, who are also the editors of this book, brought together experts from a variety of relevant fields including biological anthropology, socio-cultural anthropology, linguistics, neurology, primatology, and psychology. Revised versions of all but two of these conference papers, plus a paper on "The complementation theory of language and tool use" by Peter Reynolds, appear in the book. The resultant book is intended to facilitate the development of conceptual models of human evolution by providing "a critical assessment of current perspectives and approaches in the relevant sciences".

The book starts with two introductory chapters. First, K. Gibson provides a succinct overview of the issues, history and themes underlying the questions addressed in the forthcoming chapters. Her introduction highlights both the difficulties inherent in these questions about human evolution and the advances stemming from field and laboratory studies of animal behavior by Goodall and others. In the second introductory chapter, G. W. Hewes succinctly reviews the history of speculation on the relation between the emergence of language and tool-related skills.

The main text is divided into five sections. Part I is focused on the relationship between vision-based gesture and auditory-based vocal communication. Four chapters in this section cover: gestural use alone and with speech (Kendon), development of gestural communication in congenitally deaf children born to hearing parents (Goldin-Meadow), language competence in bonobo chimps (Savage-Rumbaugh and Rumbaugh), and comparative analysis of avian and mammalian vocal abilities (Snowdon). Snowden's critical review of non-hominoid animal communication in natural settings reveals the existence of many components of language in other species, including categorical perception and simple grammar, but finds no convincing evidence for any communication systems that approach the complexity of human language. The Rumbaughs challenge theories proposing species-specific innate language mechanisms with (1) arguments that syntax is a natural result of external constraints on language use, and (2) results from research on apes at Yerkes, including the remarkable similarity between a bonobo chimp (Kanzi) and a 2-year-old human in speech comprehension ability.

The three chapters in Part II examine the interrelationships between social behaviors and technological skills in non-human primates. Two of these chapters are dedicated to tool use in specific primate species: cebus monkeys (Visalberghi) and chimpanzees (Boesch), the latter featuring Boesch's eye-opening research on cultural transmission of nut-cracking behaviors in wild chimps. The third chapter (McGrew) compares subsistence tool use in chimps and humans, noting that chimps possess many behaviors once thought to be exclusive to humans. Only

humans, however, have been observed to independently construct tools from two or more components. As with language, human superiority in tool use and construction appears to result from an interactive convergence of abilities such as imitation and instruction in a species with high cognitive capacity.

Neurological evidence and perspectives are presented in Part III. Kempler reviews the correlated impairments of gesture and aphasia in Alzheimer's patients. Dean Falk provides brief but balanced overviews of the often controversial topics of cerebral lateralization, sex differences in visuospatial skills, and tool production and brain size in hominid evolution. Calvin argues that common neurological mechanisms underlie the complex motor sequencing and planning required by language, tool use, throwing and other behaviors. He also presents a well-reasoned account of the possible evolution of ballistic skills for which these brain mechanisms may have evolved. Finally, Gibson attempts to shed light on the evolutionary increase in brain size and cognitive ability, an increase she believes reflects the interdependent capacities for language, social behaviors, mathematical concepts, and tool use.

Part IV provides comparative developmental perspectives on language and tool behavior, with repeated consideration of heterochrony (evolutionary changes produced by change in developmental timing such as neoteny). Gibson's introduction, which suffers from a faulty analysis of neoteny (p. 275) due to a failure to distinguish physical and cognitive development, precedes a chapter by Andrew Lock that examines the intertwined development of language, tool use and concepts of causality and object permanence. Lock also provides a cautionary look at drawing inferences about evolution from development. Next, Jonas Langer compares cognitive development in macaques, cebus monkeys, and humans. Lastly in this section, Parker and Milbrath argue that language, technology and intelligence co-evolved in hominids in part as adaptations for planning. As they note, planning is an essential aspect of a wide variety of behaviors with considerable adaptive significance.

Part V contains five chapters that re-examine archaeological and fossil evidence in

light of the data from neurology, cognitive psychology, ethology, and other relevant disciplines. As Ingold notes in his introduction, the Acheulean handaxe, first crafted more than one million years ago and appearing at many sites spanning over a million years, raises the main issues addressed in this section. These include questions about cultural transmission, the causes of standardization, and implications for social behavior and communication in prehistoric hominids. Diversity of opinion is particularly clear in this section, even extending to whether the "hand axes" were themselves tools (e.g., Wynn) or merely the by-products of the production of stone flake tools (Davidson & Noble). Part V opens with an attempt by Toth and Schick to draw inferences about the minimum levels of language and cognition from the stone tools of ancient hominids. Later, Wynn (Chap. 17) casts serious doubt on whether tools or tool use provide a basis for inferences about language, but Toth and Schick are suitably cautious, drawing few conclusions while raising good questions. Davidson and Noble, clearly influenced by Toth's work, tackle the question of when language emerged. With the help of well chosen figures, they review different eras and techniques of prehistoric tools, and conclude that the evidence is consistent with a late (Upper Paleolithic) emergence of language spawned by the discovery that meaning could be conveyed by signs.

The final two chapters focus on tool use and social behavior in modern humans, arguing that the role of social interaction in the evolution of human intelligence has generally been underestimated or even neglected. Reynolds begins with the issue of continuity versus discontinuity (qualitative difference) between humans and apes, urging that we need to pay more attention to the social dimension of tool behavior. Ample examples from modern "primitives" like Australian Aborigines illustrate his point. His eye towards social aspects of tools is brought together with a distinction between gravity-independent "liths" and "polyliths" (joined liths) and "polypods" (gravity-dependent structures) in his *complementation theory*. This theory proposes that "the two most distinctive aspects of human tool use,... the heterotechnic task group and the construction of polyliths out of differentiated parts," are co-evolutionary and synergetic developments (p. 422). Taking a

broad perspective than Reynolds, Ingold criticizes attempts to explain specific skills or behaviors as outputs of particular cognitive mechanisms; rather, he argues for a view of activities such as tool use and speech as integral components of a system of perception and action arising from the complex of relationships between an agent and its environment. He distinguishes between technique (tacit, context-dependent, user skill; "knowing how") and technology (explicit, objective "knowledge that"), arguing that technique is sufficient for tool use and is typically acquired through observation and imitation rather than symbol-based communication.

The book closes with an epilogue by Ingold. Unfortunately, he declines the daunting task of synthesizing the preceding chapters into a unified, state-of-the-art, interdisciplinary view. Instead, he questions the traditional Western ways of conceptualizing language, technology and intelligence, offering a more holistic perspective in which ecological, cultural and affective context must be considered.

As in most edited books, the chapters in this book are not always consistent. In addition to the inevitable variation in matters of interpretation and inference, such as the disparate views about the implications of prehistoric tools for language, inconsistencies in 'factual' matters appear. For instance, Goldin-Meadow claims that "the spontaneous gestures of hearing individuals do not stand on their own and must be interpreted in the context of the speech they accompany" (p. 63) just a few pages after Kendon discusses "quotable gestures" which can be cited and interpreted out of context. As one might expect given the large group of diverse authors, no unified story of human evolution emerges from this volume. Nonetheless, there is general agreement on a few themes, including some degree of continuity between humans and primates. Overall, the book presents a view of language as an emergent phenomenon that develops under a complex of environmental and social conditions. Each paper also validates approaches to human origins that take seriously the interrelatedness of cognition, social behavior, tool use and language. While not integrated as well as in a typical single-author book, the

interdisciplinary connections are so numerous that the overall impact and usefulness of this book may actually be greater than the sum of the individual chapters.

While most chapters are written on a level appropriate for people from other fields, the breadth of coverage insures that experts in all relevant fields will find plenty of new food for thought. Consequently, this is a rather rare book in that it can be highly recommended for readers ranging from students to experts. All that is required is a interest in tools, language and human cognition, and this will surely be provoked by the book if not already present.

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### *A People That Shall Dwell Alone: Judaism as a Group Evolutionary Strategy*

By Kevin MacDonald. Praeger Publishers, 88 Post Rd. West, Westport, CT 06881 USA, 1994, \$57.95 (hdbk.).

Reviewed by Harmon R. Holcomb III, Dept. of Philosophy, University of Kentucky, Lexington, KY 40506-0027 USA.

MacDonald has connected the vast Jewish scholarship on the history and nature of Judaism with evolutionary theory. His scholarship is impeccable. The book is well-researched, original and insightful, and its evidence for group strategies is vast, detailed and persuasive. He skillfully employs the method of transforming apparent counterexamples to his generalizations into minor opposing trends that cannot subvert major trends.

The significance of his results, however, is hard to pin down. Eysenck's review (1995) gives the impression that the book's emphasis is on the high IQ of Jews compared to gentiles and on IQ differences between Ashkenazi and Sephardic Jews, due to natural selection of high investment parenting involving respect for education and scholarly pursuits. Hartung's review (1995) shows how one of MacDonald's theses can be derived directly from the Torah (first five books of the Bible) without recourse to Jewish history, namely, that Jewish moral codes apply only to the in-group (Jews) rather

than universally to all people (a requisite of "genuine" morality).

Not only do these reviews omit much of the content of the book, but they ignore its central concept--the idea of a group evolutionary strategy. Introducing this idea, the book is a case study that serves as an exemplar for a new way to analyze psychological and cultural phenomena in terms of evolutionary strategies for group living.

Just as an individual evolutionary strategy is a strategy for maximizing relative reproductive success of individuals, so a group evolutionary strategy is one for maximizing relative reproductive success of groups. Are we talking about group selection? No, at least, not directly. Judaism as a group strategy is not the result of natural selection at the level of the group but rather cultural inventions made possible by the ability of humans to structure groups (chapter 1). This rather subtle idea deserves some clarification.

The bane of group selectionism has been the possible existence of selfish cheaters within the group. Humans are uniquely intelligent among species, permitting a novel kind of selection, one that does not require genetic differences that underlie ratios between within-group and between-group fitness effects (as does D.S. Wilson's theory, Wilson and Sober, 1994). Instead, any number of us could get together, identify ourselves as a group, and think up a strategy to outcompete other groups by regulating behavior toward in-group and out-group members. We could enact social controls to prevent defection, cheating, and freeloading by raising their costs (e.g., through punishment consisting of lowered social status of family members--Boyd and Richerson 1985). There is no theoretical reason to rule out evolutionary strategies as conscious choices among human groups that bypass the usual restrictions on genetic group selection. This idea is consonant with the usual theory of humans as flexible strategists who learn from their experiences and traditions how to attain genetically fixed evolutionary goals.

In the case of the Jews, conscious social controls of this sort have led them to outcompete other groups and to avoid the twin dangers of military annihilation and cultural assimilation that led to the demise of so many groups throughout history. MacDonald shows

how Jewish norms segregate Jews both culturally and genetically from non-Jews. The key Jewish norm of marrying intelligent Jews (assortative, consanguineous, endogamous marriage) has set up genetic conditions that potentially permit natural selection acting on Jews at the level of the group. Thus, Judaism exhibits a number of conscious social controls that qualify it as a group evolutionary strategy without being the product of group natural selection. The results of these psychological and cultural causes, however, involve genetic differences, among Jews and between Jews and gentiles, that are potentially subject to both individual and group selection.

MacDonald is a pluralist about whether individual and group interests prevail. He describes competition for mates and social and economic status as examples of within-group conflict in which individual interests triumph over group interests. He regards the history of responses to external threats to Jews as examples of the coincidence of group and individual interests. He argues that community control of reproduction and a high level of altruism within the group are examples of the triumph of group interests over individual interests. Within-group individual competition is combined with sanctions that do not allow individuals to maximize their interests to the detriment of Jewish community.

The various chapters of the book delineate aspects of the group evolutionary strategy that defines Judaism primarily as a people (nation/ethnic group) and only secondarily as a religion. The group strategy preserves genetic integrity by structuring marriages and preventing significant levels of proselytism (chapters 2, 3, 4). It engages Jews and gentiles in resource competition (chapter 5). It highly structures relationships among Jews and between Jews and gentiles (chapter 6). It promotes eugenic practices that resulted in a high degree of specialization for intelligence and high investment parenting (chapter 7), and is made possible by evolved domain-general psychological mechanisms, e.g., conscientiousness, high affect intensity in individuals, and ethnocentrism (also chapter 7). It originated as a collectivist culture in which people with these psychological predispositions utilized the experience of the Egyptian sojourn as a basis for interpreting their history and constructing their group evolutionary strategy (chapter 8).

For example, Jewish ideology (chapters 3, 4) includes a number of cultural isolating mechanisms. The Tanakh (Old Testament) advises Jews to control resources, exterminate other peoples to avoid intermarriage, idealize endogamy and racial purity, honor polygyny (Solomon was said to have "seven hundred wives, princesses, and three hundred multiply." Not only do these norms enable them to compete reproductively with other groups, but the ideology of the separateness of the Jews guides their behavior over history. The Exodus from Egypt, not the Creation, was their key event. Persecution for worshipping an Israelite God when sojourning in foreign lands enhanced their group identification. Oppression signaled that the Jew had been unfaithful to their God. Those who kept the faith and obeyed the religious regulations enjoyed a high level of reproductive success, whereas those who did not paid for their crime with lowered reproductive success. Throughout Jewish history, Jewish norms have created "a large overlap among scholarship, control of economic resources, social status, and, ultimately, fertility" (p. 186) that makes sense only in the light of evolution. As a result, Jews around the world are more genetically and culturally related to other Jews than to the peoples in their host societies.

MacDonald makes an admirable attempt to interpret Judaism in an objective, unbiased, scientific manner without lapsing into either anti-semitism or Jewish apologetics (for his discussion of anti-Semitism, see his companion volume, 1995). However, he is prone to regarding Jewish ideology and religion as nothing more than an irrational instrument for rationalization of its group evolutionary functions. Here are seven examples:

(1) "For the Israelites, there was really only one purpose for God-- to represent the idea of kinship, in-group membership, and separateness from others" (p. 45).

(2) "In Chapter 3, it was suggested that monotheism for the Israelites was nothing more or less than an expression of the common interests of the Jewish people viewed as a unified kinship group. In a sense, therefore, one can equate the monotheistic God, the interests of a unified Israel, and the interests of the Levites and particularly the priestly

descendants of Aaron" (p. 252).

(3) "The ideology is non-falsifiable (and thus self-perpetuating) because it explains both success and failure in terms that imply continued allegiance to the group. Moreover, since adversity is always attributed to failure to obey religious practices, blame is always internalized. The result is to prevent a rational appraisal of the reasons for the adversity by examining the Israelites' behavior vis-à-vis their neighbors" (p. 49).

(4) "The sojourning ideology of the Tanakh is simply a rationalization of a previously existing powerful tendency toward endogamy, consanguinity, and ethnocentrism" (p. 249).

(5) "From an evolutionary perspective, in the absence of actual genetic assimilation one is left to conclude that this Jewish sense of moral and religious idealism, which results in genetic segregation, is in fact a mask for a self-interested evolutionary strategy aimed at promoting the interests of a kinship group that maintains its genetic integrity during a diaspora" (p. 64).

(6) (on studying the Jewish religious canon:) "Despite the logical veneer, the point was not to make a rational, scholarly argument. A great deal of intelligence was required, but ultimately there was no attempt to seek truth, religious or otherwise. These writings are thus ultimately irrational. And as is inevitable with irrational undertakings, acceptance of the Jewish canon was essentially an act of authoritarian submission" (p. 176).

(7) "... in fact, 'being different' is in some sense what Judaism is all about..." (p. 196).

MacDonald would deny anti-semitism or anti-clericalism in such apparently derogatory comments, since they express his view of human nature generally. He holds that "... the evolutionist is also keenly aware of the ways in which our ideologies can rationalize our self-serving behavior. And, in a very real sense, we cannot afford to continue to hide our heads in the sand while ethnic conflict continues to escalate" (pp. 1f).

Even so, we go too far when we pass quickly from statements of the form "the



evolutionary cause/function of x is y" (as in quotation 1) to statements of the form "x is nothing more or less than an expression of y" (quotation 2), or to "one can equate x with y" (quotation 2), or to "x prevents rational appraisal" (quotation 3), or to "x is simply a rationalization of y" (quotation 4), or to "x is a mask for y" (quotation 5), or to "x is irrational" (quotation 6), or to "y is what x is all about" (quotation 7). The error lies in ignoring the way evolutionary theory's domain places limits on evolutionary conclusions (see Holcomb, 1993), and in confusing evolutionary explanations with determinations of rational justifiability. To imply that evolutionary self-interest explains everything about humans and trumps the rationality of what we think taken on its own terms is to deny standard nonevolutionary views their legitimacy and rationality. That illogical "nothing-but-ism" invites rebuke.

Despite the preceding excesses, the central idea of a group evolutionary strategy as an experiment in living involving conscious choices of peoples who construct their own groups should prove useful to evolutionists. MacDonald has shown how the detailed patterns of Jewish history provide evidence for a host of evolutionary hypotheses about group strategies that apply to other groups as well. The theoretical pluralism and evidential meticulousness of this volume makes it a case study that repays careful reading.

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### *The Moral Animal: Evolutionary Psychology and Everyday Life*

By Robert Wright. Pantheon Books, 201 E. 50th St., New York, NY 10022 USA, 1994, \$27.50 (hdbk), also in ppr.

Reviewed by Hector Qirko. Department of Anthropology, University of Tennessee, Knoxville, TN 37996, USA.

William Irons (1995) recently characterized the term "evolutionary psychology" as the latest attempt to transform sociobiology into something palatable to the general public. To the extent that this is true, no one has contributed more to the process than Robert Wright, a science and philosophy writer who has published two recent cover stories on the subject for *Time* (1994, 1995), and who in 1994 produced *The Moral Animal*.

Evolutionary psychology focuses on cognitive adaptations presumed to underlie human behavior. Involved in it are not only psychologists, but also evolutionary biologists, human ethologists, and anthropologists, among others. It is a subset of the new Darwinian paradigm, the current attempt to understand human behavior in Darwinian terms. That Wright often conflates "evolutionary psychology" and "the Darwinian paradigm," while ignoring much relevant psychological research (e.g., research on current adaptiveness), is a problem at the outset.

The book is divided into four sections. Part one deals with "Sex, romance, and love" and focuses on sex differences with respect to mate choice. Part two, "Social cement," deals primarily with kin selection and reciprocal altruism. Part three, "Social strife," touches upon the quest for status and the role of deception (including self-deception) in human relationships. Utilizing examples from research on human and other species as well as his own anecdotal observations, Wright introduces lay readers to the "second

[Darwinian] level of human nature," as well as to its direct relevance in their lives.

In these three sections, unfortunately Wright undermines his own credibility through his rather dramatic and, in some instances imprecise, language. For example, in discussing parental investment and the role of stepparents, Wright concludes: "Fathers give their children all kinds of tutelage and guidance...and guard them against all kinds of threats...A mother alone simply can't pick up the slack. A stepfather almost surely won't pick up much, if any of it. In Darwinian terms, a young stepchild is an obstacle to fitness, a drain on resources" (pp. 103f). This is a far cry from Daly and Wilson, whose statistics on child abuse and murder he cites and who are, quite properly, much more cautious: "Substitute parents will generally tend to care less profoundly for children than natural parents" (p. 103).

As can be seen in the above example, Wright appears to believe that what is true of a group will be true of each of its members. This is seen again in his use of Darwin's personal life to illustrate findings in evolutionary psychology. For example, through family letters we learn the details of Darwin's courtship and marriage. Wright then explains the parties' thoughts, desires, and actions in evolutionary psychological terms. But the manifestation of human cognitive mechanisms can be confounded by individual genetic variability, developmental history, socialization, etc. The use of particular individuals to illustrate them is therefore problematic at best.

Also disappointing is Wright's lack of analysis in his final section on morality and ethics. To the fundamental question Can morality have no meaning for the thinking person in a post-Darwinian world? he responds: "This is a deep and murky question that (readers may be relieved to hear) will not be rigorously addressed in this book" (p. 329). He does suggest, based on the writings of John Stuart Mill, that we can build a new morality on the idea that everyone's happiness counts equally. But this approach has failed often and miserably in a pre-Darwinian world (e.g., the Christian "golden rule"), and so is a particularly unsatisfying recommendation.

Given our tendencies to be nepotistic, perhaps an ethical system designed to extend and reinforce kinship ties to include the planet (as we already extend them to some degree through

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concepts such as nationhood) would be more realistic. Certainly many scholars have given the question Wright raises a great deal of attention (e.g., Alexander, 1987; Provine, 1988; Wilson, 1984). In ignoring more current and studied perspectives, *The Moral Animal*'s "moral" component is sadly lacking.

All in all, I find the book a strange combination of loose review, personal biography, and informal moralizing not likely to succeed in clearly articulating either evolutionary psychology or the Darwinian paradigm to its intended readers.

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### *The Descent of the Child: Human Evolution From a New Perspective*

by Elaine Morgan. Oxford University Press, 200 Madison Avenue, New York, NY 10016 USA, 1995, \$20 (hdbk.)

### *The Scars of Evolution: What Our Bodies Tell Us about Human Origins*

By Elaine Morgan. Souvenir Press Ltd., 43 Great Russell Street, London WC1B 3PA, 1990, \$20 (hdbk.) and Oxford University Press, see supra, 1993, \$12 (ppr.).

Reviewed by Dorothy Tennov, RR 9, Box 251, Millsboro, Delaware 19966, USA

Charles Darwin explained why we are so similar to apes and other mammals -- we are related. Elaine Morgan attacks the mystery of why we are so unlike them. In the two books reviewed here she elaborates and expands on the theory of hominid nature and origins that has come to be referred to by the term she used in her first book (Morgan, 1982) devoted to the subject, "aquatic ape theory" (henceforth, AAT).

The spark that ignited Morgan's interest in human origins came from a talk in 1960 by marine biologist Alister Hardy whose curiosity had been aroused by the layers of fat he found in whales, porpoises, and other sea-going creatures, a trait found in *Homo sapiens* but not in other primates. What he called then a "highly ingenious (and unlikely) theory" was based on the speculation that "the original terrestrial hominid" underwent a sustained semi-aquatic existence.

Conventional wisdom holds that major steps from apes to hominids were adaptations

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to a savannah environment, but certain "awkward facts" do not square with this view. Distinctly human characteristics are not found in other savannah primates which are known to have moved from forest to grasslands. Why did we, and not baboons or vervets, lose body hair and become bipedal? According to AAT, some five to eight million years ago the area known as Afar was subjected to massive sea flooding. Hominids in that area would have been forced to adapt to a semi-aquatic life. Afar is the site where the oldest hominid fossils have been found. The environment, which varied between times of plenty and times of hardship, was conducive to rapid evolutionary change. Those who would become our ancestors fled the arid mainland to the seashore where they fed on sea creatures and escaped the heat.

It is hard to imagine bipedalism as an adaptation on the savannah. Unless instantly efficient, developing bipedalists would have been vulnerable to predators. Concerning the need for free hands for carrying (a Savannah Theory cause of upright gait), chimps and gorillas manage quite well to hold food in one arm while walking with three. Nor were weapons or other tools the cause; it is now known that hominids walked on two legs long before they used tools. AAT posits that it was during the aquatic phase that body hair, which is not protective when wet and is a handicap in the water, was lost except for the less-often submerged head. The subcutaneous layer of fat is good for keeping both warm and buoyant and occurs in some aquatic mammals, and differs from the fat distribution pattern on other primates living today. Keeping one's head above water is difficult when on all fours. *Homo sapiens* also differs from apes but is similar to marine species in having spine-lower limb parallelism, pelvic rotation, and voluntary breath control (a prerequisite speech).

Thus far, AAT and the questions it attacks have largely been ignored by established scientists. Morgan points out that, on the issue of why humans lost their body hair, others have argued that no explanation is called for, that we may never know the reason, or even that there may not be a reason, attitudes she calls not merely defeatist, but fundamentally unscientific. AAT has been

satirized (Symons, 1984), and called "eccentric" (Kingdon, 1993), "exaggerated" (Corballis, 1991), an "entertaining fantasy", "sensationalist" (Eldridge & Tattersal, 1991), "a mixture of fact, folklore, and fiction" (Claiborne, 1974), and "an unlikely oddity" (Poirier, 1973). But while paleontologists are accustomed to basing their theories on hard evidence exhumed painstakingly from ancient rocks, the search for human origins need not be limited to analysis of remnants of the footprints and body parts of creatures long deceased. Speculation about human origins can also be based on DNA, on comparative anatomy with extant animals and with other fossils, and on examination of existing organisms themselves.

The story of AAT, which was described a decade ago as a "brief flourish," is perhaps just beginning. Although many recent books in anthropology and paleontology afford it not a word, this probably says more about academia and the human mind than about the value of AAT. All good science is speculation at the outset, especially speculation that synthesizes. In summary, two advantages to AAT stand out: (1) It exemplifies a type of scientific speculation that combines and makes sense of a wide range of disparate and previously unconnected facts. (2) It raises new questions--questions that within the insularity of the separate fields had not previously been given serious consideration. Mere absence of confirmation is not a reasonable criticism if an idea is consistent with known facts. AAT may not be supported by the fossil record, but neither is it in opposition to it, dealing, as it does, with events presumed to have taken place during a period notably poor in fossils.

Like Randolph Nesse and George Williams in *Why We Get Sick*, Morgan in *The Scars of Evolution* considers the unfortunate consequences of evolutionary processes which operate solely on the basis of reproductive competition. The result is a body that works, but in some respects only barely; it is vulnerable to ills and ailments that are the by-products of an unplanned natural selection process. Sunburn, birthing malfunction, acne, hemorrhoids, and the slow healing and greater vulnerability to infection of open wounds are the "prices we pay for being human," the "scars" left by our evolutionary past. Both

books consider the fact that the position of the larynx beside the windpipe increases the risk of choking. Both also deal with the universal lower back pain that results from displacement of the spine from the original horizontal position of tetrapods. While Nesse and Williams focus on the value of an evolutionary approach to illness, however, Morgan's thinking is primarily directed at the processes by which various human characteristics may have come into being. She also conjectures about relationships between an aquatic history and: loss of estrus, frontal-frontal copulation, and brain development.

Because Morgan moves from physiology to social behavior, *The Descent of the Child* might appropriately be placed in the sociology section in the library as well as in the section on evolution. Even readers adamantly opposed to the idea that early hominids were subject to natural selection under conditions of coastal living might find value in the manner in which Morgan discusses the contemporary social quandaries of overpopulation, child care arrangements, and the state of the family. As the labile, rare, and not well-established nuclear family structure breaks down, as caretaker and child are isolated together for many hours a day, and as infant behavior is incompatible with adult lifestyles, the conditions of childhood have changed in ways that raise new problems. Morgan views having two parents as good for the child, and stresses the child's ability to relate well to more than one caretaker, but she worries about unwanted children under the care of the undertrained and unwilling. She notes that a woman who gives birth today may never have cared for a baby, nor even have witnessed breast feeding. That care of a newborn must be learned is attested to by observations of maternal behavior in primates, including humans.

There are a few admonitions. References are meager for such a wide-ranging presentation, fewer than 100, and notes are completely absent. It would be helpful if Morgan published her notes separately for scholars. Are some of the "facts" adduced in building the AAT picture of human evolution less well-supported than others? Morgan is not a formally trained scientist. She read literature at Oxford and earned her living as a dramatist for the British Broadcasting

Company.

With all that said, if you read Morgan's books be prepared for an unusual treat. As a popular science writer, Morgan does what few of us are gifted to do. She expresses her ideas in a manner so palatable that she may deceive those accustomed to the exactitude demanded by journal editors into thinking that what she offers is less than profound. As one reviewer said, Morgan writes with "wit and facility which the rest of us can only regard with awe." These are books that can be recommended to friends as well as to students. It is hard to imagine a better vehicle for immersing a lay person in how evolutionary thinking relates to all aspects of human living.

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## *Why We Get Sick : The New Science of Darwinian Medicine*

By Randolph M. Nesse and George C. Williams. Times Books, 201 E. 50th St., New York, NY 10022 USA, 1995, \$24 (hdbk), \$13 (ppr.).

Reviewed by Thomas L. Shellberg, Henry Ford College, 5101 Evergreen, Dearborn, MI 48128, USA.

Among the endorsements and praises on the back cover of this marvelous book, Robert Ornstein says, "Every so often a book comes along that has the power to change the way we live and die. This splendid book is one, and it could well revolutionize the way physicians are taught, the way they practice, and even the way parents watch over their child with a fever or cough." E. O. Wilson says, "By bringing the evolutionary vision systematically into one of the last unconquered provinces, Nesse and Williams have devised not only the means for the improvement of medicine but, equally important, fundamental new insights into the human condition."

These are strong claims, but one could justify even more effusive praise. *Why We Get Sick* is a profoundly important book, partly because it is so revolutionary. It is the first book to systematically and comprehensively apply evolutionary thinking to questions of sickness and health--the first to provide a solid Darwinian framework for ultimately understanding *why* illness and disease exist at all, and *why* symptoms and defects and responses are the way they are--what they *mean*.. Medical science has been largely limited heretofore to proximate explanations, mostly unaware of the potential of modern evolutionary theory to guide doctors and medical theorists to ask the right sorts of questions to understand illness itself and dramatically improve treatment.

Nesse and Williams do a wonderful job in suggesting those kinds of questions which need to be asked from an evolutionary perspective. Which symptoms have evolved as adaptive defenses, such as fever or sequestration of iron during infections, and

which are manipulations which have evolved in the best interests of spreading the pathogen? This makes a big difference in how and whether the symptoms should be treated. How should we think about allergic responses, myopia, cancer, anxiety disorders, morning sickness, gestational diabetes, hypertension, heel spurs, cold symptoms, obesity, and the meanings of pain? Which of our problems are due to natural toxins and which occur because of novel environments or design-flaw legacies, or are diseases of civilization? Which of them reflect natural defenses and which do not? Which disorders do genes underlie and why do these genes exist? How can medicine be improved by an understanding of evolutionary arms races?

All this and much more are discussed in a well organized, sensible taxonomy of essential questions and perspectives attached to a solid Darwinian base. This book will likely be a pre-eminent first source in this field for many years to come.

The writing is wonderful: clear, interesting, stimulating, exploratory, undogmatic, informal, and broadly appealing, with lots of practical information. Most of the book can be understood without previous knowledge of medicine or selection theory. Despite the casual style and popular appeal of this book, though, even sophisticated evolutionary theorists and medical doctors will find it very valuable. It is scientifically careful and responsible, and rich with thoughtful suggestions for future research. It would make an ideal text, too; I think it ought to be required reading for all first-year medical students.

So many topics are explored, from alcoholism to the evolution of virulence, that some readers will surely disagree with some perspectives and conclusions. For example, I believe that Williams' pleiotropic theory of senescence is incorrect, and I've told him why, but it really doesn't matter if every branch in this ambitious book is not equally strong. Nesse and Williams have provided a trunk so solid and healthy it will give rise to abundant new growth for a very long time.

Richard Dawkins is also quoted on the back cover. He says, "Buy two copies and give one to your doctor." I suggest you buy *three* copies and also give one to your local biology teacher. Few biology teachers, even the half of them or so who include evolution in their courses, realize the power of selection theory for answering "why" questions about biological phenomena. Despite the revolution in understanding behavior which has occurred because of the application of modern selection theory, there has been little impact on biology teaching. Most biology teachers don't teach about behavior and they couldn't tell you who Hamilton and Williams are or even the difference between proximate and ultimate explanations. Many are quite uncomfortable when it comes to talk of human behavior and evolution. But they do teach anatomy and physiology and most do believe that it's important for students to understand questions about health and disease. *Why We Get Sick* might just do the trick. Of all the contemporary books on evolution, I suspect this one has the greatest potential to demonstrate to biology teachers the power of modern selection theory for answering "why" questions and to thus stimulate improvement in biology teaching.

Maybe someone should send a copy to Pat Buchanan, too. If he read it when he was seriously sick, who knows? He might be born yet again and stop railing against teaching evolution in the public schools.

## ANNOUNCEMENTS

### AESVICO

The third world congress of the International Association for Scientific Exchange on Violence and Human Coexistence is scheduled for 17-21 August 1996 at University College, Dublin. One of the suggested section themes is "Darwinian approaches to the future of society." For information contact Prof. Don Bennett, Secretariat of the IIIrd World Congress of AESVICO, University College Dublin, Dublin 4, Ireland.

### *Economic Aspects of Behavior*

The Jean-Marie Delwart Foundation will conduct its international seminar in Louvain-la-Neuve, Belgium 18-20 October 1996. The topic will be Economic Aspects of Behavior in Animals and Man. The seminar is being organized with the intention of reviewing our current understanding of how animals, including Man, come to make decisions; to what extent these conform with economic optimality; and the mechanisms underlying the (pseudo)rational choices made. Both ultimate and proximate aspects will be considered. Speakers will be: P. Hammerstein (Seewiesen), R. Dunbar (Liverpool), E. Ostrom (Indiana University), A. Pomiankowski (London), E. Voland (Giessen), K. Lessells (Leiden University), R. A. Hinde (Cambridge), R. Noé (Seewiesen), G. Parker (Liverpool), J. van Hooff (Utrecht), A. Dhondt (Cornell University), and J. Van Alphen (Leiden). For information, contact Raphaëlle Holender, Fondation Jean-Marie Delwart, Château de Pellenberg, B-3212 Belgium, tél. 32-16-46-04-03, fax 32-16-46-39-75.

### *Delwart Foundation Award for Human Ethology and Cultural Anthropology*

The Jean-Marie Delwart Foundation will award a prize for a research, or series of researchers, individual or collective, on the cooperation and organization of productive activities, more specifically on the comparative analysis of economic aspects of animal and human behavior. The prize of \$10,000 will be given for works written or translated in French or English. Submissions should be sent by 15 February 1997 to: Fondation Jean-Marie Delwart, Château de Pellenberg, B-3212 Pellenberg, Belgique. Include an application letter, curriculum vitae (-arum), and complete list of publications. The jury consists of members of the scientific committee of the Foundation and of members of the Académie Royale des Sciences de Belgique.

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## Obituary: Donald T. Campbell

We regret to have to announce that ISHB member Donald T. Campbell died 7 May 1996 at age 79 of complications from surgery for colon cancer. Trained in social psychology at Berkeley, Dr. Campbell had faculty listings at Lehigh University in psychology, anthropology, sociology, and education. He proposed what he called evolutionary epistemology as a unifying theory of knowledge, and studied bias, prejudice, and erroneous theories. His thesis, coauthored by Donald W. Fiske, "Convergent and discriminant validation by the multitrait-multimethod approach," may be the most frequently cited paper in social science. He also studied tribal peoples of East Africa, and Head Start programs in the US.

## Congress for Applied Psychology

The International Congress for Applied Psychology has issued the call for papers for its convention to be held in San Francisco, California 9-14 August 1996. Hosted by the American Psychological Association for the International Association of Applied Psychology, the Congress will feature presentations on organizational psychology, psychological evaluation and assessment, psychology and national development, applied gerontology, health psychology, economic psychology, psychology and law, political psychology, sport psychology, traffic and transportation psychology, and other areas. For a copy of the call, contact Congress Secretariat, APA Office of International Affairs, 750 First St. NE, Washington, DC 20002-4242 USA, fax 1-202-336-5956, e-mail icap@apa.org.

## Computer Literature Searches

It has been suggested that ISHE members would find it useful to use Sociofile instead of Psychlit when searching for references on a particular topic. Sociofile includes the journal *Ethology and Sociobiology*, plus much cross-cultural literature not featured in Psychlit.



## CURRENT LITERATURE

June 1996

Interested in possibly reviewing one of the books below or some other suitable book? Please contact the appropriate book review editor (see Editorial Staff box).

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